SPENSER (EDMUND)—*Continued*
etti," "Epithalamion," "Foure Hymnes," "Daphnaida," "Complaints," and "Muipotmos."

"Prosopopoia" is dated 1613, but this is apparently the earliest date in folio. Indeed this piece is described as a separate book in the "Dictionary of National Biography."

STEVENSON (ROBERT LOUIS). ON THE THERMAL Influence of Forests. By Robert Louis Stevenson. 8vo, original blue wrappers, uncut. $45.00.

Edinburgh, *Printed by Neill and Company*, 1873.

* Very fine copy of the first separate impression, with the title repeated on the front cover and without the legend, "From the Proceedings of the Royal Society of Edinburgh, Vol. VIII, 1872-73."

This essay was reprinted twice in the same year (1873). Stevenson's bibliographer, Col. W. F. Prideaux, accepts the above as the earlier form.

STEVENSON (ROBERT LOUIS). ON THE THERMAL Influence of Forests. 8vo, original blue wrappers. $15.00.

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STEVENSON (ROBERT LOUIS). MORAL EMBLEMS A Second Collection of Cuts and Verses. 24mo, unbound. $80.00.

Printers: S. L. Osbourne & Company, Davos-Platz, [1882].

* Very rare.

STEVENSON (ROBERT LOUIS). UNWIN'S ANNUAL, 1886. The Broken Shaft. Tales in Mid-Ocean. Edited by Henry Norman. 4to, original wrappers. $2.50.

London, *T. Fisher Unwin*, 1886.

* Contains first edition of "Markheim." By R. L. S. Though dated 1886 this Annual was published in December, 1885.

STEVENSON (ROBERT LOUIS). KIDNAPPED BEING Memoirs of the Adventures of David Balfour in the Year 1751. By Robert Louis Stevenson. 12mo, original cloth. $3.50.

London, *Cassell and Co.*, 1886.

* First published edition.

Dodd & Livingston
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ON THE

THERMAL INFLUENCE OF FORESTS.

BY

ROBERT LOUIS STEVENSON, Esq.

EDINBURGH:
PRINTED BY NEILL AND COMPANY.
MDCCCLXXIII.

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The opportunity of an experiment on a comparatively large scale, and under conditions of comparative isolation, can occur but rarely in such a science as Meteorology. Hence Mr Milne Home's proposal for the plantation of Malta seemed to offer an exceptional opportunity for progress. Many of the conditions are favourable to the simplicity of the result; and it seemed natural that, if a searching and systematic series of observations were to be immediately set afoot, and continued during the course of the plantation and the growth of the wood, some light would be thrown on the still doubtful question of the climatic influence of forests.

Mr Milne Home expects, as I gather, a threefold result:—1st, an increased and better regulated supply of available water; 2d, an increased rainfall; and, 3d, a more equable climate, with more temperate summer heat and winter cold.* As to the first of these expectations, I suppose there can be no doubt that it is justified by facts; but it may not be unnecessary to guard against any confusion of the first with the second. Not only does the presence of growing timber increase and regulate the supply of running and spring water independently of any change in the amount of rainfall, but, as Boussingault found at Marmato,† denudation of forest is sufficient to decrease that supply, even when the rainfall has increased instead of diminished in amount. The second and third effects stand apart, therefore, from any question as to the utility of Mr Milne Home's important proposal; they are both,

* Journal Scot. Met. Soc., New Series, No. xxvi., p. 35.

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perhaps, worthy of discussion at the present time, but I wish to confine myself in the present paper to the examination of the third alone.

A wood, then, may be regarded either as a *superficies* or as a *solid*; that is, either as a part of the earth's surface slightly elevated above the rest, or as a diffused and heterogeneous body displacing a certain portion of free and mobile atmosphere. It is primarily in the first character that it attracts our attention, as a radiating and absorbing surface, exposed to the sun and the currents of the air; such that, if we imagine a plateau of meadow-land or bare earth raised to the mean level of the forest's exposed leaf-surface, we shall have an agent entirely similar in kind, although perhaps widely differing in the amount of action. Now, by comparing a tract of wood with such a plateau as we have just supposed, we shall arrive at a clear idea of the specialities of the former. In the first place, then, the mass of foliage may be expected to increase the radiating power of each tree. The upper leaves radiate freely towards the stars and the cold inter-stellar spaces, while the lower ones radiate to those above and receive less heat in return; consequently, during the absence of the sun, each tree cools gradually downward from top to bottom. Hence we must take into account not merely the area of leaf-surface actually exposed to the sky, but, to a greater or less extent, the surface of every leaf in the whole tree or the whole wood. This is evidently a point in which the action of the forest may be expected to differ from that of the meadow or naked earth; for though, of course, inferior strata tend to a certain extent to follow somewhat the same course as the mass of inferior leaves, they do so to a less degree—conduction, and the conduction of a very slow conductor, being substituted for radiation.

We come next, however, to a second point of difference. In the case of the meadow, the chilled air continues to lie upon the surface, the grass, as Humboldt says, remaining all night submerged in the stratum of lowest temperature; while in the case of trees, the coldest air is continually passing down to the space underneath the boughs, or what we may perhaps term the crypt of the forest. Here it is that the consideration of any piece of woodland conceived as a solid comes naturally in; for this solid contains a portion of the atmosphere, partially cut off from the rest, more or less excluded from the influence of wind, and lying upon a soil

that is screened all day from insolation by the impending mass of foliage. In this way (and chiefly, I think, from the exclusion of winds), we have underneath the radiating leaf-surface a stratum of comparatively stagnant air, protected from many sudden variations of temperature, and tending only slowly to bring itself into equilibrium with the more general changes that take place in the free atmosphere.

Over and above what has been mentioned, thermal effects have been attributed to the vital activity of the leaves in the transudation of water, and even to the respiration and circulation of living wood. The whole actual amount of thermal influence, however, is so small that I may rest satisfied with mere mention. If these actions have any effect at all, it must be practically insensible; and the others that I have already stated are not only sufficient validly to account for all the observed differences, but would lead naturally to the expectation of differences very much larger and better marked. To these observations I proceed at once. Experience has been acquired upon the following three points:—1. The relation between the temperature of the trunk of a tree and the temperature of the surrounding atmosphere; 2. The relation between the temperature of the air under a wood and the temperature of the air outside; and, 3. The relation between the temperature of the air above a wood and the temperature of the air above cleared land.

As to the first question, there are several independent series of observations; and I may remark in passing, what applies to all, that allowance must be made throughout for some factor of specific heat. The results were as follows:—The seasonal and monthly means in the tree and in the air were not sensibly different. The variations in the tree, in M. Becquerel's own observations, appear as considerably less than a fourth of those in the atmosphere, and he has calculated, from observations made at Geneva between 1796 and 1798, that the variations in the tree were less than a fifth of those in the air; but the tree in this case, besides being of a different species, was seven or eight inches thicker than the one experimented on by himself.* The variations in the tree, therefore,

* Atlas Météorologique de l'Observatoire Impérial, 1867.

are always less than those in the air, the ratio between the two depending apparently on the thickness of the tree in question and the rapidity with which the variations followed upon one another. The times of the maxima, moreover, were widely different: in the air, the maximum occurs at 2 P.M. in winter, and at 3 P.M. in summer; in the tree, it occurs in winter at 6 P.M., and in summer between 10 and 11 P.M. At nine in the morning in the month of June, the temperatures of the tree and of the air had come to an equilibrium. A similar difference of progression is visible in the means, which differ most in spring and autumn, and tend to equalise themselves in winter and in summer. But it appears most strikingly in the case of variations somewhat longer in period than the daily ranges. The following temperatures occurred during M. Becquerel's observations in the Jardin des Plantes:—

1859.

Date.	Temperature of the Air.	Temperature in the Tree.
Dec. 15,	26°·78	32°
„ 16,	19·76	32
„ 17,	17·78	31·46
„ 18,	13·28	30·56
„ 19,	12·02	28·40
„ 20,	12·54	25·34
„ 21,	38·30	27·86
„ 22,	43·34	30·92
„ 23,	44·06	31·46

A moment's comparison of the two columns will make the principle apparent. The temperature of the air falls nearly fifteen degrees in five days; the temperature of the tree, sluggishly following, falls in the same time less than four degrees. Between the 19th and the 20th the temperature of the air has changed its direction of motion, and risen nearly a degree; but the temperature of the tree persists in its former course, and continues to fall nearly three degrees farther. On the 21st there comes a sudden increase of heat, a sudden thaw; the temperature of the air rises twenty-five and a-half degrees; the change at last reaches the tree,

but only raises its temperature by less than three degrees; and even two days afterwards, when the air is already twelve degrees above freezing point, the tree is still half a degree below it. Take, again, the following case:—

1859.

Date.	Temperature of the Air.	Temperature in the Tree.
July 13,	84°·92	76°·28
„ 14,	82·58	78·62
„ 15,	80·42	77·72
„ 16,	79·88	78·44
„ 17,	73·22	75·92
„ 18,	68·54	74·30
„ 19,	65·66	70·70

The same order reappears. From the 13th to the 19th the temperature of the air steadily falls, while the temperature of the tree continues apparently to follow the course of previous variations, and does not really begin to fall, is not really affected by the ebb of heat, until the 17th, three days at least after it had been operating in the air.* Hence we may conclude that all variations of the temperature of the air, whatever be their period, from twenty-four hours up to twelvemonths, are followed in the same manner by variations in the temperature of the tree; and that those in the tree are always less in amount and considerably slower of occurrence than those in the air. This *thermal sluggishness*, so to speak, seems capable of explaining all the phenomena of the case without any hypothetical vital power of resisting temperatures below the freezing point, such as is hinted at even by Becquerel.

Reaumur, indeed, is said to have observed temperatures in slender trees nearly thirty degrees higher than the temperature of the air in the sun; but we are not informed as to the conditions under which this observation was made, and it is therefore impossible to assign to it its proper value. The sap of the ice-plant is said to be

* Comptes Rendus de l'Académie, 29th March 1869.

materially colder than the surrounding atmosphere ; and there are several other somewhat incongruous facts, which tend, at first sight, to favour the view of some inherent power of resistance in some plants to high temperatures, and in others to low temperatures.* But such a supposition seems in the meantime to be gratuitous. Keeping in view the thermal redispositions, which must be greatly favoured by the ascent of the sap, and the difference between the condition as to temperature of such parts as the root, the heart of the trunk, and the extreme foliage, and never forgetting the unknown factor of specific heat, we may still regard it as possible to account for all anomalies without the aid of any such hypothesis. We may, therefore, I think, disregard small exceptions, and state the result as follows :—

If, after every rise or fall, the temperature of the air remained stationary for a length of time proportional to the amount of the change, it seems probable—setting aside all question of vital heat —that the temperature of the tree would always finally equalise itself with the new temperature of the air, and that the range in tree and atmosphere would thus become the same. This pause, however, does not occur : the variations follow each other without interval ; and the slow-conducting wood is never allowed enough time to overtake the rapid changes of the more sensitive air. Hence, so far as we can see at present, trees appear to be simply bad conductors, and to have no more influence upon the temperature of their surroundings than is fully accounted for by the consequent tardiness of their thermal variations.

Observations bearing on the second of the three points have been made by Becquerel in France, by La Cour in Jutland and Iceland, and by Rivoli at Posen. The results are perfectly congruous. Becquerel's observations † were made under wood, and about a hundred yards outside in open ground, at three stations in the district of Montargis, Loiret. There was a difference of more than one degree Fahrenheit between the mean annual temperatures in favour of the open ground. The mean summer temperature in the wood was from two to three degrees lower than the mean sum-

* Prof Balfour's Class-Book of Botany, Physiology, chap. xii. page 670.
† Comptes Rendus, 1867 and 1869.

mer temperature outside. The mean maxima in the wood were also lower than those without by a little more than two degrees. Herr La Cour* found the daily range consistently smaller inside the wood than outside. As far as regards the mean winter temperatures, there is an excess in favour of the forest, but so trifling in amount as to be unworthy of much consideration. Libri found that the minimum winter temperatures were not sensibly lower at Florence, after the Appenines had been denuded of forest, than they had been before.† The disheartening contradictoriness of his observations on this subject led Herr Rivoli to the following ingenious and satisfactory comparison.‡ Arranging his results according to the wind that blew on the day of observation, he set against each other the variation of the temperature under wood from that without, and the variation of the temperature of the wind from the local mean for the month :—

Wind, . .	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Var. in Wood,	+0·60	+0·26	+0·26	+0·04	−0·04	−0·20	+0·16	+0·07
Var. in Wind,	−0·30	−2·60	−3·30	−1·20	+1·00	+1·30	+1·00	+1·00

From this curious comparison, it becomes apparent that the variations of the difference in question depend upon the amount of variations of temperature which take place in the free air, and on the slowness with which such changes are communicated to the stagnant atmosphere of woods; in other words, as Herr Rivoli boldly formulates it, a forest is simply a bad conductor. But this is precisely the same conclusion as we have already arrived at with regard to individual trees; and in Herr Rivoli's table, what we see is just another case of what we saw in M. Becquerel's—the different progression of temperatures. It must be obvious, however, that the thermal condition of a single tree must be different in many ways from that of a combination of trees and more or less

* See his paper.

† Annales de Chimie et de Physique, xlv., 1830. A more detailed comparison of the climates in question would be a most interesting and important contribution to the subject.

‡ Reviewed in the Austrian Meteorological Magazine, vol. iv. p. 543.

stagnant air, such as we call a forest. And accordingly we find, in the case of the latter, the following new feature: The mean yearly temperature of woods is lower than the mean yearly temperature of free air, while they are decidedly colder in summer, and very little, if at all, warmer in winter. Hence, on the whole, forests are colder than cleared lands. But this is just what might have been expected from the amount of evaporation, the continued descent of cold air, and its stagnation in the close and sunless crypt of a forest; and one can only wonder here, as elsewhere, that the resultant difference is so insignificant and doubtful.

We come now to the third point in question, the thermal influence of woods upon the air above them. It will be remembered that we have seen reason to believe their effect to be similar to that of certain other surfaces, except in so far as it may be altered, in the case of the forest, by the greater extent of effective radiating area, and by the possibility of generating a descending cold current as well as an ascending hot one. M. Becquerel is (so far as I can learn) the only observer who has taken up the elucidation of this subject. He placed his thermometers at three points:* A and B were both about seventy feet above the surface of the ground; but A was at the summit of a chestnut tree, while B was in the free air, fifty feet away from the other. C was four or five feet above the ground, with a northern exposure; there was also a fourth station to the south, at the same level as this last, but its readings are very seldom referred to. After several years of observation, the mean temperature at A was found to be between one and two degrees higher than that at B. The order of progression of differences is as instructive here as in the two former investigations. The maximum difference in favour of station A occurred between three and five in the afternoon, later or sooner according as there had been more or less sunshine, and ranged sometimes as high as seven degrees. After this the difference kept declining until sunrise, when there was often a difference of a degree, or a degree and a half, upon the other side. On cloudy days the difference tended to a minimum. During a rainy month of April, for example, the difference in favour of station A was less than half a

* Comptes Rendus, 28th May 1860.

degree; the first fifteen days of May following, however, were sunny, and the difference rose to more than a degree and a half.* It will be observed that I have omitted up to the present point all mention of station C. I do so because M. Becquerel's language leaves it doubtful whether the observations made at this station are logically comparable with those made at the other two. If the end in view were to compare the progression of temperatures above the earth, above a tree, and in free air, removed from all such radiative and absorptive influences, it is plain that all three should have been equally exposed to the sun or kept equally in shadow. As the observations were made, they give us no notion of the relative action of earth-surface and forest-surface upon the temperature of the contiguous atmosphere; and this, as it seems to me, was just the *crux* of the problem. So far, however, as they go, they seem to justify the view that all these actions are the same in kind, however they may differ in degree. We find the forest heating the air during the day, and heating it more or less according as there has been more or less sunshine for it to absorb, and we find it also chilling it during the night; both of which are actions common to any radiating surface, and would be produced, if with differences of amount and time, by any other such surface raised to the mean level of the exposed foliage.

To recapitulate:

1*st*, We find that single trees appear to act simply as bad conductors.

2*d*, We find that woods, regarded as solids, are, on the whole, slightly lower in temperature than the free air which they have displaced, and that they tend slowly to adapt themselves to the various thermal changes that take place without them.

3*d*, We find forests regarded as surfaces acting like any other part of the earth's surface, probably with more or less difference in amount and progression, which we still lack the information necessary to estimate.

All this done, I am afraid that there can be little doubt that the more general climatic investigations will be long and vexatious. Even in South America, with extremely favourable conditions, the

* Comptes Rendus, 20th May 1861.

result is far from being definite. Glancing over the table published by M. Becquerel in his book on climates, from the observations of Humboldt, Hall, Boussingault, and others, it becomes evident, I think, that nothing can be founded upon the comparisons therein instituted ; that all reasoning, in the present state of our information, is premature and unreliable. Strong statements have certainly been made ; and particular cases lend themselves to the formation of hasty judgments. "From the Bay of Cupica to the Gulf of Guayaquil," says M. Boussingault, "the country is covered with immense forests and traversed by numerous rivers ; it rains there almost ceaselessly ; and the mean temperature of this moist district scarcely reaches 78°·8 F. . . . . . At Payta commence the sandy deserts of Priura and Sechura ; to the constant humidity of Choco succeeds almost at once an extreme of dryness ; and the mean temperature of the coast increases at the same time by 1°·8 F."* Even in this selected favourable instance it might be argued that the part performed in the change by the presence or absence of forest was comparatively small ; there seems to have been, at the same time, an entire change of soil ; and, in our present ignorance, it would be difficult to say by how much this of itself is able to affect the climate. Moreover, it is possible that the humidity of the one district is due to other causes besides the presence of wood, or even that the presence of wood is itself only an effect of some more general difference or combination of differences. Be that as it may, however, we have only to look a little longer at the table before referred to, to see how little weight can be laid on such special instances. Let us take five stations, all in this very district of Choco. Hacquita is eight hundred and twenty feet above Novita, and their mean temperatures are the same. Alto de Mombu, again, is five hundred feet higher than Hacquita, and the mean temperature has here fallen nearly two degrees. Go up another five hundred feet to Tambo de la Orquita, and again we find no fall in the mean temperature. Go up some five hundred further to Chami, and there is a fall in the mean temperature of nearly six degrees. Such numbers are evidently quite untrustworthy ; and hence we may judge

* Becquerel, "Climats," p. 141.

how much confidence can be placed in any generalisation from these South American mean temperatures.

The question is probably considered too simply—too much to the neglect of concurrent influences. Until we know, for example, somewhat more of the comparative radiant powers of different soils, we cannot expect any very definite result. A change of temperature would certainly be effected by the plantation of such a marshy district as the Sologne, because, if nothing else were done, the roots might pierce the impenetrable subsoil, allow the surface-water to drain itself off, and thus dry the country. But might not the change be quite different if the soil planted were a shifting sand, which, *fixed* by the roots of the trees, would become gradually covered with a vegetable earth, and thus be changed from dry to wet? Again, the complication and conflict of effects arises, not only from the soil, vegetation, and geographical position of the place of the experiment itself, but from the distribution of similar or different conditions in its immediate neighbourhood, and probably to great distances on every side. A forest, for example, as we know from Herr Rivoli's comparison, would exercise a perfectly different influence in a cold country subject to warm winds, and in a warm country subject to cold winds; so that our question might meet with different solutions even on the east and west coasts of Great Britain.

The consideration of such a complexity points more and more to the plantation of Malta as an occasion of special importance; its insular position and the unity of its geological structure both tend to simplify the question. There are certain points about the existing climate, moreover, which seem specially calculated to throw the influence of woods into a strong relief. Thus, during four summer months, there is practically no rainfall. Thus, again, the northerly winds when stormy, and especially in winter, tend to depress the temperature very suddenly; and thus, too, the southerly and south-westerly winds, which raise the temperature during their prevalence to from eighty-eight to ninety-eight degrees, seldom last longer than a few hours; insomuch that "their disagreeable heat and dryness may be escaped by carefully closing the windows and doors of apartments at their onset."* Such sudden and short

* Scoresby-Jackson's "Medical Climatology."

variations seem just what is wanted to accentuate the differences in question. Accordingly, the opportunity seems one not lightly to be lost, and the British Association or this Society itself might take the matter up and establish a series of observations, to be continued during the next few years. Such a combination of favourable circumstances may not occur again for years; and when the whole subject is at a stand-still for want of facts, the present occasion ought not to go past unimproved.

Such observations might include the following :—

The observation of maximum and minimum thermometers in three different classes of situation—*videlicet*, in the areas selected for plantation themselves, at places in the immediate neighbourhood of those areas where the external influence might be expected to reach its maximum, and at places distant from those areas where the influence might be expected to be least.

The observation of rain-gauges and hygrometers at the same three descriptions of locality.

In addition to the ordinary hours of observation, special readings of the thermometers should be made as often as possible at a change of wind and throughout the course of the short hot breezes alluded to already, in order to admit of the recognition and extension of Herr Rivoli's comparison.

Observation of the periods and forces of the land and sea breezes.

Gauging of the principal springs, both in the neighbourhood of the areas of plantation and at places far removed from those areas.

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ON THE

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BY

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From the Proceedings of the Royal Society of Edinburgh,
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„ 17,	17·78	31·46
„ 18,	13·28	30·56
„ 19,	12·02	28·40
„ 20,	12·54	25·34
„ 21,	38·30	27·86
„ 22,	43·34	30·92
„ 23,	44·06	31·46

A moment's comparison of the two columns will make the principle apparent. The temperature of the air falls nearly fifteen degrees in five days; the temperature of the tree, sluggishly following, falls in the same time less than four degrees. Between the 19th and the 20th the temperature of the air has changed its direction of motion, and risen nearly a degree; but the temperature of the tree persists in its former course, and continues to fall nearly three degrees farther. On the 21st there comes a sudden increase of heat, a sudden thaw; the temperature of the air rises twenty-five and a-half degrees; the change at last reaches the tree,

but only raises its temperature by less than three degrees; and even two days afterwards, when the air is already twelve degrees above freezing point, the tree is still half a degree below it. Take, again, the following case :—

1859.

Date.	Temperature of the Air. °	Temperature in the Tree. °
July 13,	84·92	76·28
„ 14,	82·58	78·62
„ 15,	80·42	77·72
„ 16,	79·88	78·44
„ 17,	73·22	75·92
„ 18,	68·54	74·30
„ 19,	65·66	70·70

The same order reappears. From the 13th to the 19th the temperature of the air steadily falls, while the temperature of the tree continues apparently to follow the course of previous variations, and does not really begin to fall, is not really affected by the ebb of heat, until the 17th, three days at least after it had been operating in the air.* Hence we may conclude that all variations of the temperature of the air, whatever be their period, from twenty-four hours up to twelve months, are followed in the same manner by variations in the temperature of the tree; and that those in the tree are always less in amount and considerably slower of occurrence than those in the air. This *thermal sluggishness*, so to speak, seems capable of explaining all the phenomena of the case without any hypothetical vital power of resisting temperatures below the freezing point, such as is hinted at even by Becquerel.

Reaumur, indeed, is said to have observed temperatures in slender trees nearly thirty degrees higher than the temperature of the air in the sun; but we are not informed as to the conditions under which this observation was made, and it is therefore impossible to assign to it its proper value. The sap of the ice-plant is said to be

* Comptes Rendus de l'Academie, 29th March 1869.

materially colder than the surrounding atmosphere; and there are several other somewhat incongruous facts, which tend, at first sight, to favour the view of some inherent power of resistance in some plants to high temperatures, and in others to low temperatures.* But such a supposition seems in the meantime to be gratuitous. Keeping in view the thermal redispositions, which must be greatly favoured by the ascent of the sap, and the difference between the condition as to temperature of such parts as the root, the heart of the trunk, and the extreme foliage, and never forgetting the unknown factor of specific heat, we may still regard it as possible to account for all anomalies without the aid of any such hypothesis. We may, therefore, I think, disregard small exceptions, and state the result as follows:—

If, after every rise or fall, the temperature of the air remained stationary for a length of time proportional to the amount of the change, it seems probable—setting aside all question of vital heat—that the temperature of the tree would always finally equalise itself with the new temperature of the air, and that the range in tree and atmosphere would thus become the same. This pause, however, does not occur: the variations follow each other without interval; and the slow-conducting wood is never allowed enough time to overtake the rapid changes of the more sensitive air. Hence, so far as we can see at present, trees appear to be simply bad conductors, and to have no more influence upon the temperature of their surroundings than is fully accounted for by the consequent tardiness of their thermal variations.

Observations bearing on the second of the three points have been made by Becquerel in France, by La Cour in Jutland and Iceland, and by Rivoli at Posen. The results are perfectly congruous. Becquerel's observations † were made under wood, and about a hundred yards outside in open ground, at three stations in the district of Montargis, Loiret. There was a difference of more than one degree Fahrenheit between the mean annual temperatures in favour of the open ground. The mean summer temperature in the wood was from two to three degrees lower than the mean sum-

* Prof. Balfour's Class-Book of Botany, Physiology, chap. xii. page 670.
† Comptes Rendus, 1867 and 1869.

mer temperature outside. The mean maxima in the wood were also lower than those without by a little more than two degrees. Herr La Cour * found the daily range consistently smaller inside the wood than outside. As far as regards the mean winter temperatures, there is an excess in favour of the forest, but so trifling in amount as to be unworthy of much consideration. Libri found that the minimum winter temperatures were not sensibly lower at Florence, after the Appenines had been denuded of forest, than they had been before.† The disheartening contradictoriness of his observations on this subject led Herr Rivoli to the following ingenious and satisfactory comparison.‡ Arranging his results according to the wind that blew on the day of observation, he set against each other the variation of the temperature under wood from that without, and the variation of the temperature of the wind from the local mean for the month :—

Wind, . .	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Var. in Wood,	+0·60	+0·26	+0·26	+0·04	–0·04	–0·20	+0·16	+0·07
Var. in Wind,	–0·30	–2·60	–3·30	–1·20	+1·00	+1·30	+1·00	+1·00

From this curious comparison, it becomes apparent that the variations of the difference in question depend upon the amount of variations of temperature which take place in the free air, and on the slowness with which such changes are communicated to the stagnant atmosphere of woods; in other words, as Herr Rivoli boldly formulates it, a forest is simply a bad conductor. But this is precisely the same conclusion as we have already arrived at with regard to individual trees; and in Herr Rivoli's table, what we see is just another case of what we saw in M. Becquerel's—the different progression of temperatures. It must be obvious, however, that the thermal condition of a single tree must be different in many ways from that of a combination of trees and more or less

* See his paper.

† Annales de Chimie et de Physique, xlv., 1830. A more detailed comparison of the climates in question would be a most interesting and important contribution to the subject.

‡ Reviewed in the Austrian Meteorological Magazine, vol. iv. p. 543.

stagnant air, such as we call a forest. And accordingly we find, in the case of the latter, the following new feature: The mean yearly temperature of woods is lower than the mean yearly temperature of free air, while they are decidedly colder in summer, and very little, if at all, warmer in winter. Hence, on the whole, forests are colder than cleared lands. But this is just what might have been expected from the amount of evaporation, the continued descent of cold air, and its stagnation in the close and sunless crypt of a forest; and one can only wonder here, as elsewhere, that the resultant difference is so insignificant and doubtful.

We come now to the third point in question, the thermal influence of woods upon the air above them. It will be remembered that we have seen reason to believe their effect to be similar to that of certain other surfaces, except in so far as it may be altered, in the case of the forest, by the greater extent of effective radiating area, and by the possibility of generating a descending cold current as well as an ascending hot one. M. Becquerel is (so far as I can learn) the only observer who has taken up the elucidation of this subject. He placed his thermometers at three points: * A and B were both about seventy feet above the surface of the ground; but A was at the summit of a chestnut tree, while B was in the free air, fifty feet away from the other. C was four or five feet above the ground, with a northern exposure; there was also a fourth station to the south, at the same level as this last, but its readings are very seldom referred to. After several years of observation, the mean temperature at A was found to be between one and two degrees higher than that at B. The order of progression of differences is as instructive here as in the two former investigations. The maximum difference in favour of station A occurred between three and five in the afternoon, later or sooner according as there had been more or less sunshine, and ranged sometimes as high as seven degrees. After this the difference kept declining until sunrise, when there was often a difference of a degree, or a degree and a half, upon the other side. On cloudy days the difference tended to a minimum. During a rainy month of April, for example, the difference in favour of station A was less than half a

* Comptes Rendus, 28th May 1860.

degree; the first fifteen days of May following, however, were sunny, and the difference rose to more than a degree and a half.* It will be observed that I have omitted up to the present point all mention of station C. I do so because M. Becquerel's language leaves it doubtful whether the observations made at this station are logically comparable with those made at the other two. If the end in view were to compare the progression of temperatures above the earth, above a tree, and in free air, removed from all such radiative and absorptive influences, it is plain that all three should have been equally exposed to the sun or kept equally in shadow. As the observations were made, they give us no notion of the relative action of earth-surface and forest-surface upon the temperature of the contiguous atmosphere; and this, as it seems to me, was just the *crux* of the problem. So far, however, as they go, they seem to justify the view that all these actions are the same in kind, however they may differ in degree. We find the forest heating the air during the day, and heating it more or less according as there has been more or less sunshine for it to absorb, and we find it also chilling it during the night; both of which are actions common to any radiating surface, and would be produced, if with differences of amount and time, by any other such surface raised to the mean level of the exposed foliage.

To recapitulate:

1*st*, We find that single trees appear to act simply as bad conductors.

2*d*, We find that woods, regarded as solids, are, on the whole, slightly lower in temperature than the free air which they have displaced, and that they tend slowly to adapt themselves to the various thermal changes that take place without them.

3*d*, We find forests regarded as surfaces acting like any other part of the earth's surface, probably with more or less difference in amount and progression, which we still lack the information necessary to estimate.

All this done, I am afraid that there can be little doubt that the more general climatic investigations will be long and vexatious. Even in South America, with extremely favourable conditions, the

* Comptes Rendus, 20th May 1861.

result is far from being definite. Glancing over the table published by M. Becquerel in his book on climates, from the observations of Humboldt, Hall, Boussingault, and others, it becomes evident, I think, that nothing can be founded upon the comparisons therein instituted; that all reasoning, in the present state of our information, is premature and unreliable. Strong statements have certainly been made; and particular cases lend themselves to the formation of hasty judgments. "From the Bay of Cupica to the Gulf of Guayaquil," says M. Boussingault, "the country is covered with immense forests and traversed by numerous rivers; it rains there almost ceaselessly; and the mean temperature of this moist district scarcely reaches 78°·8 F. . . . . . At Payta commence the sandy deserts of Priura and Sechura; to the constant humidity of Choco succeeds almost at once an extreme of dryness; and the mean temperature of the coast increases at the same time by 1°·8 F." * Even in this selected favourable instance it might be argued that the part performed in the change by the presence or absence of forest was comparatively small; there seems to have been, at the same time, an entire change of soil; and, in our present ignorance, it would be difficult to say by how much this of itself is able to affect the climate. Moreover, it is possible that the humidity of the one district is due to other causes besides the presence of wood, or even that the presence of wood is itself only an effect of some more general difference or combination of differences. Be that as it may, however, we have only to look a little longer at the table before referred to, to see how little weight can be laid on such special instances. Let us take five stations, all in this very district of Choco. Hacquita is eight hundred and twenty feet above Novita, and their mean temperatures are the same. Alto de Mombu, again, is five hundred feet higher than Hacquita, and the mean temperature has here fallen nearly two degrees. Go up another five hundred feet to Tambo de la Orquita, and again we find no fall in the mean temperature. Go up some five hundred further to Chami, and there is a fall in the mean temperature of nearly six degrees. Such numbers are evidently quite untrustworthy; and hence we may judge

* Becquerel, "Climats," p. 141.

how much confidence can be placed in any generalisation from these South American mean temperatures.

The question is probably considered too simply—too much to the neglect of concurrent influences. Until we know, for example, somewhat more of the comparative radiant powers of different soils, we cannot expect any very definite result. A change of temperature would certainly be effected by the plantation of such a marshy district as the Sologne, because, if nothing else were done, the roots might pierce the impenetrable subsoil, allow the surface-water to drain itself off, and thus dry the country. But might not the change be quite different if the soil planted were a shifting sand, which, *fixed* by the roots of the trees, would become gradually covered with a vegetable earth, and thus be changed from dry to wet? Again, the complication and conflict of effects arises, not only from the soil, vegetation, and geographical position of the place of the experiment itself, but from the distribution of similar or different conditions in its immediate neighbourhood, and probably to great distances on every side. A forest, for example, as we know from Herr Rivoli's comparison, would exercise a perfectly different influence in a cold country subject to warm winds, and in a warm country subject to cold winds; so that our question might meet with different solutions even on the east and west coasts of Great Britain.

The consideration of such a complexity points more and more to the plantation of Malta as an occasion of special importance; its insular position and the unity of its geological structure both tend to simplify the question. There are certain points about the existing climate, moreover, which seem specially calculated to throw the influence of woods into a strong relief. Thus, during four summer months, there is practically no rainfall. Thus, again, the northerly winds when stormy, and especially in winter, tend to depress the temperature very suddenly; and thus, too, the southerly and south-westerly winds, which raise the temperature during their prevalence to from eighty-eight to ninety-eight degrees, seldom last longer than a few hours; insomuch that "their disagreeable heat and dryness may be escaped by carefully closing the windows and doors of apartments at their onset."* Such sudden and shor

* Scoresby-Jackson's "Medical Climatology."

variations seem just what is wanted to accentuate the differences in question. Accordingly, the opportunity seems one not lightly to be lost, and the British Association or this Society itself might take the matter up and establish a series of observations, to be continued during the next few years. Such a combination of favourable circumstances may not occur again for years; and when the whole subject is at a stand-still for want of facts, the present occasion ought not to go past unimproved.

Such observations might include the following :—

The observation of maximum and minimum thermometers in three different classes of situation—*videlicet,* in the areas selected for plantation themselves, at places in the immediate neighbourhood of those areas where the external influence might be expected to reach its maximum, and at places distant from those areas where the influence might be expected to be least.

The observation of rain-gauges and hygrometers at the same three descriptions of locality.

In addition to the ordinary hours of observation, special readings of the thermometers should be made as often as possible at a change of wind and throughout the course of the short hot breezes alluded to already, in order to admit of the recognition and extension of Herr Rivoli's comparison.

Observation of the periods and forces of the land and sea breezes.

Gauging of the principal springs, both in the neighbourhood of the areas of plantation and at places far removed from those areas.